CORRESPONDENCE

Because we receive many more letters than we have room to publish we may shorten those that we do publish to allow readers as wide a selection as possible. In particular, when we receive several letters on the same topic we reserve the right to abridge individual letters. Our usual policy is to reserve our correspondence columns for letters commenting on issues discussed recently (within six weeks) in the BMJ.

Letters critical of a paper may be sent to the authors of the paper so that their reply may appear in the same issue. We may also forward letters that we decide not to publish to the authors of the paper on which they comment.

Letters should not exceed 400 words and should be typed double spaced and signed by all authors, who should include their main degree.

Proposal to outlaw the term "negative trial"

SIR,—Minerva seems somewhat ambivalent about negative trials. In 1983 she thought that they "have never made riveting reading,"[1] while more recently she thought that they "are (almost) always worth putting on record" (23 February, p 644). She has come round to the right way of thinking of course, although she might have added that there is no such thing as a "negative trial." All trials that have been well conceived and well conducted[2]—whatever their results—represent positive contributions to knowledge.

I suspect that the concept of a negative trial derives from an inflated reverence for differences between trial groups which achieve some rather arbitrarily chosen level of statistical significance. The adverse consequences of this phenomenon were recognised long ago,[3] and Dr J Stuart Pocock recently made excellent suggestions for confronting it (5 January, p 39-42). One important implication of Dr Pocock's recommendations is that investigators, referees, and editors should not exercise favouritism in respect of those trials which have results which they regard as "positive."

The magnitude of the "selective publication bias" which results from editorial designation of trials as negative or positive is unknown, so there is currently no basis for dismissing it as unlikely to be important. My colleagues and I would like to try to assess the extent of selective publication bias in perinatal medicine. We have established what we believe to be a fairly complete register of published reports of controlled trials in perinatal medicine which have appeared since 1950.

We would be most grateful to anyone who could let us know about any perinatal trials which have been conducted over this period but which have never appeared in print. By comparing the results of published and unpublished trials we would hope to obtain some estimate of the extent to which diligent readers of published reports are being misled by the selective suppression both of negative trials and possibly of "positive trials" which challenge prevailing hypotheses (and are thus perceived in some powerful quarters as negative in quite a different sense).

It is particularly important to derive estimates of the magnitude of selective publication biases if the exciting new possibilities presented by "meta-analyses" of data pooled from independently mounted, but similar, trials are to be exploited in a manner which is as scientifically rigorous as possible.

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1 Minerva. Views. *Br Med J* 1983;**287**:1886.
2 Mosteller F, Gilbert JP, McPeek B. Reporting standards and research strategies for controlled trials: agenda for the editor. *Controlled Clinical Trials* 1980;**1**:37-58.
3 Walster W, Clearly TA. A proposal for a new editorial policy in the Social Sciences. *The American Statistician* 1970;April:16-8.

Prolactinomas

SIR,—We feel that Dr A Grossman and Professor G M Besser (19 January, p 182) have taken an overpessimistic view of the value of surgery in patients with prolactinomas. Their success rate, using radiotherapy, as judged by restoration of serum prolactin to normal in "about one third,"[1] is certainly substantially worse than surgery and on long term follow up may reveal a higher rate of hypopituitarism.

For large prolactinomas we agree with Bergh *et al* that bromocriptine is the treatment of choice to reduce prolactin, induce fertility, and decrease tumour size.[2] Thereafter it can be used to control possible expansion in pregnancy in the knowledge that the drug has no adverse effects on the fetus.[3]

The authors' objections to surgery appear to be based on one paper by Serri *et al*,[4] which gives a recurrence rate of 50% after surgery for microadenomas. It should be pointed out, however, that these results were obtained in an early series of cases, when surgery for prolactinomas was in its infancy and the importance of a wide excision of the tumour was not appreciated. It is not surprising that limited removal in the form of "selective adenomectomy" as practised in the series of Serri *et al* should result in some recurrences. We are of the opinion that after a decision to treat surgically has been made the correct operation for prolactinomas is a partial hypophysectomy which includes a wide margin of normal tissue around the tumour rather than selective microadenomectomy. In our series of patients in whom normoprolactinaemia was restored by partial hypophysectomy (26 out of 35) there have been no recurrences up to a maximum of five years among patients with microadenomas (10) or macroadenomas (16) (paper in preparation). Although our follow up period is not yet five years for all patients, our findings contrast strikingly with those of Serri and colleagues.

We consider that Dr Grossman and Professor Besser have taken too gloomy a view of surgery and we agree with Teasdale *et al* that transsphenoidal surgery for prolactino-